

SCIENCE.

FRIDAY, SEPTEMBER 10, 1886.

COMMENT AND CRITICISM.

THE SMITHSONIAN REPORT for 1885, which we may hope will be issued with less delay than its predecessors have been, will contain an account of the progress in astronomy for that year, by Mr. William C. Winlock of Washington, which has already appeared with sufficient promptness as a separatum. Mr. Winlock forestalls at once any criticism we might otherwise like to make by pleading the brief time necessarily available as an excuse for any shortcomings that may be found, and remarks that his record is intended primarily for the large and increasing class of those who have a general rather than a special interest in the progress of astronomy, while it may be of use to the professional astronomer also, as a convenient collection of reviews and notes. Abstracts of the most important papers are given, while other papers appear by title only, and free use has been made of reviews in such periodicals as *Science*, *The athenaeum*, *The observatory*, and *Bulletin astronomique*. Comets, a specialty of Mr. Winlock's, are very fully and accurately dealt with; and his method of indicating the names of all these objects, now become so numerous with every year, is an important advance.

Independently of the excellences or shortcomings of the present work, we think the question may fairly be raised whether these annual reports are worthy of continuance or not. They are, through no fault of the author, rather tame reading for those having only a general interest in astronomy, being largely a mere recital of the new facts of the year's finding out, with no connecting-link to the astronomy of the past. To be sure, the developments of astronomy within a twelvemonth are rarely sufficiently far-reaching for even the practical astronomer to keep in mind the precise relations of past and present research. Again, if these reports are prepared for the convenience of the professional astronomer, it may well be doubted whether they are worth what they cost the astronomer who undertakes to prepare them; for the work is no ap-

proach, in point of serviceableness, to a complete bibliography for the year, such, in fact, as Mr. Winlock himself broaches the preparation of, perhaps through the co-operation of astronomers. If this is found practicable, then the editor of the Smithsonian report might well confine himself to the presentation of a quinquennial history of astronomical progress, to be prepared by the ablest astronomer who would undertake the task, and who would be expected to indicate clearly the bearings of recent research upon that of previous years, and weld the scattering links into a continuous chain. It is easy to see that the work executed in this manner would have an important bearing upon 'the diffusion of knowledge among men,' which, in its present form, it does not possess.

JUDGING BY THE SCIENTIFIC AGITATION which has shaken England for so many years, one would hardly credit the statement made by Sir John Lubbock in his address at the unveiling of the statue of the founder of the Mason science college, that, in 54 of 240 endowed schools for boys which have reported, no science whatever is taught; in 50, one hour is devoted to it per week; in 76, less than three hours; while only 56 devoted as many as six hours to it. According to the report of the Technical commission last year, there were only three schools in Great Britain in which science is fully and adequately taught. In urging the benefits of science, Sir John Lubbock says, "In the first place, science adds immensely to the interest and happiness of life. It is altogether a mistake to regard science as dry or prosaic. The technical works, descriptions of species, etc., bear the same relations to science as dictionaries to literature. . . . Occasionally, indeed, it may destroy some poetical myth of antiquity, such as the ancient Hindoo explanation of rivers, that 'Indra dug out their beds with his thunderbolts, and sent them forth by long continuous paths.' But the real causes of natural phenomena are far more striking, and contain more real poetry, than those which have occurred to the untrained imagination of mankind."

DR. THOMAS TAYLOR'S MICROSCOPIC METHOD for detecting the adulteration of butter with foreign

fats seems destined to assume as many shapes as Proteus. At first the globose forms, obtained by the boiling and subsequent slow cooling of butter, and exhibiting the Saint Andrew's cross under polarized light, were brought prominently forward as distinguishing marks of pure butter. Prof. H. H. Weber, however, upon testing the method as described by Dr. Taylor, found, that, although the so-called butter crystals could be readily prepared from butter, they could be as readily prepared from beef-fat, or mixtures of beef-fat and lard, under like conditions. The necessary conditions are, the slow cooling of the melted fat in the presence of minute solid particles about which the fat may crystallize, the so-called 'butter crystals' being aggregations of minute crystals radiating from a centre. In the test as described by Dr. Taylor, the butter is boiled for one minute, and then slowly cooled. During the boiling, some of the water of the butter evaporates, and a corresponding portion of its salt solidifies, and the minute crystals thus formed serve as centres of crystallization for the fat during the subsequent cooling.

After the publication of these results, the 'butter crystal' and its Saint Andrew's cross were relegated to a subordinate position, and in several publications Dr. Taylor insisted that his most important test had been neglected, viz., the appearance of the unboiled material under polarized light with a selenite plate. According to Dr. Taylor, butter shows a uniform tint, while lard and tallow show prismatic colors. Here, again, however, he has been pursued by Professor Weber, who shows that either butter-fat or lard or tallow, when cooled quickly, will show a uniform tint, while if cooled slowly, so as to admit of the formation of larger crystals, prismatic tints are shown by both. Since imitation butter is cooled rapidly when made, and since both genuine and imitation butter are liable to undergo sufficient changes of temperature after manufacture to allow of a partial re-crystallization, the test is plainly fallacious. Apparently, Dr. Taylor prepared his annual report with these results in mind, for there, and in his paper before the annual meeting of the American society of microscopists at Chautauqua, Aug. 10-16, he gives his method a still different exposition.

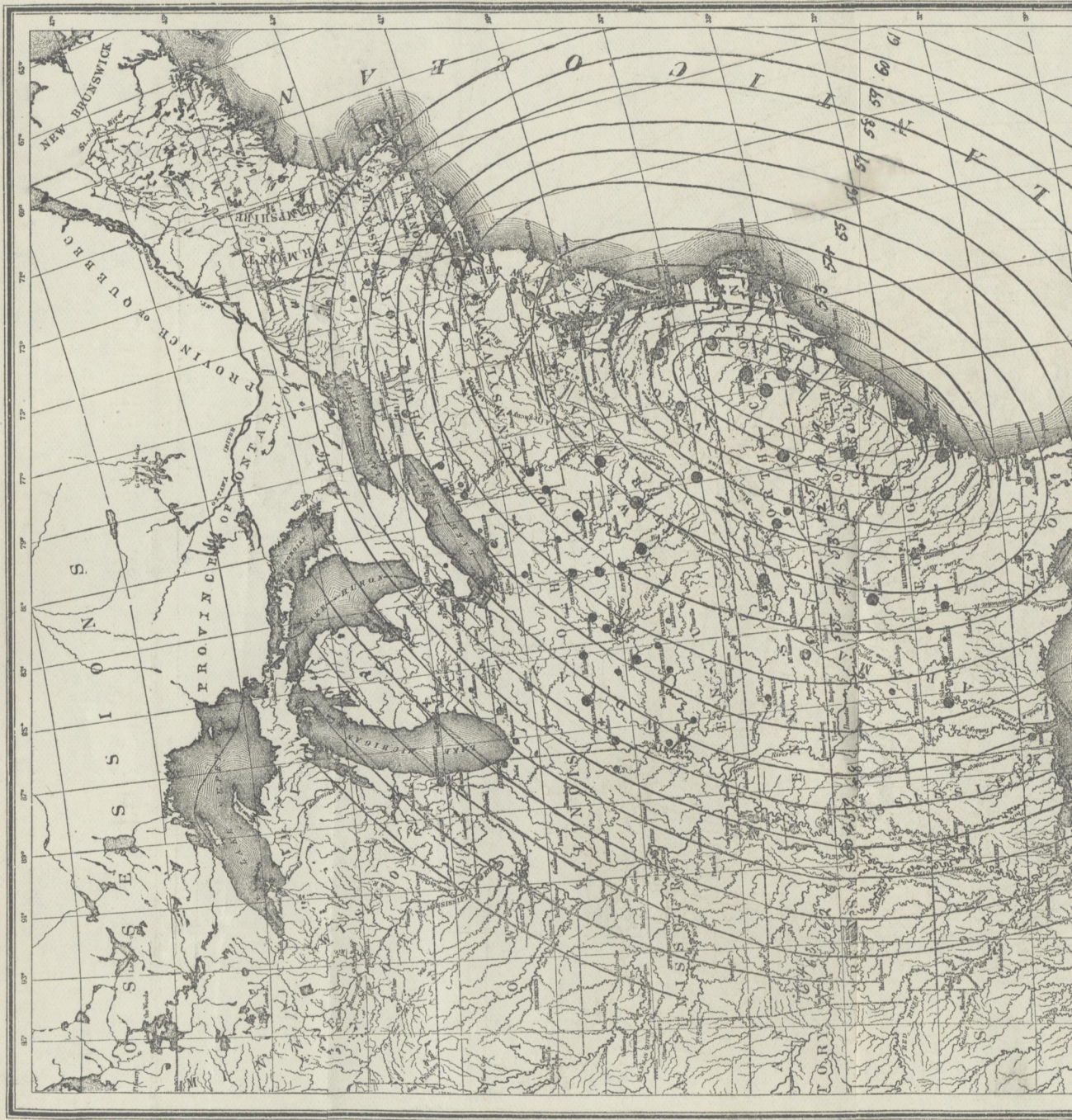
Dr. Taylor's first step is now to search for fat crystals in the test sample by plain transmitted

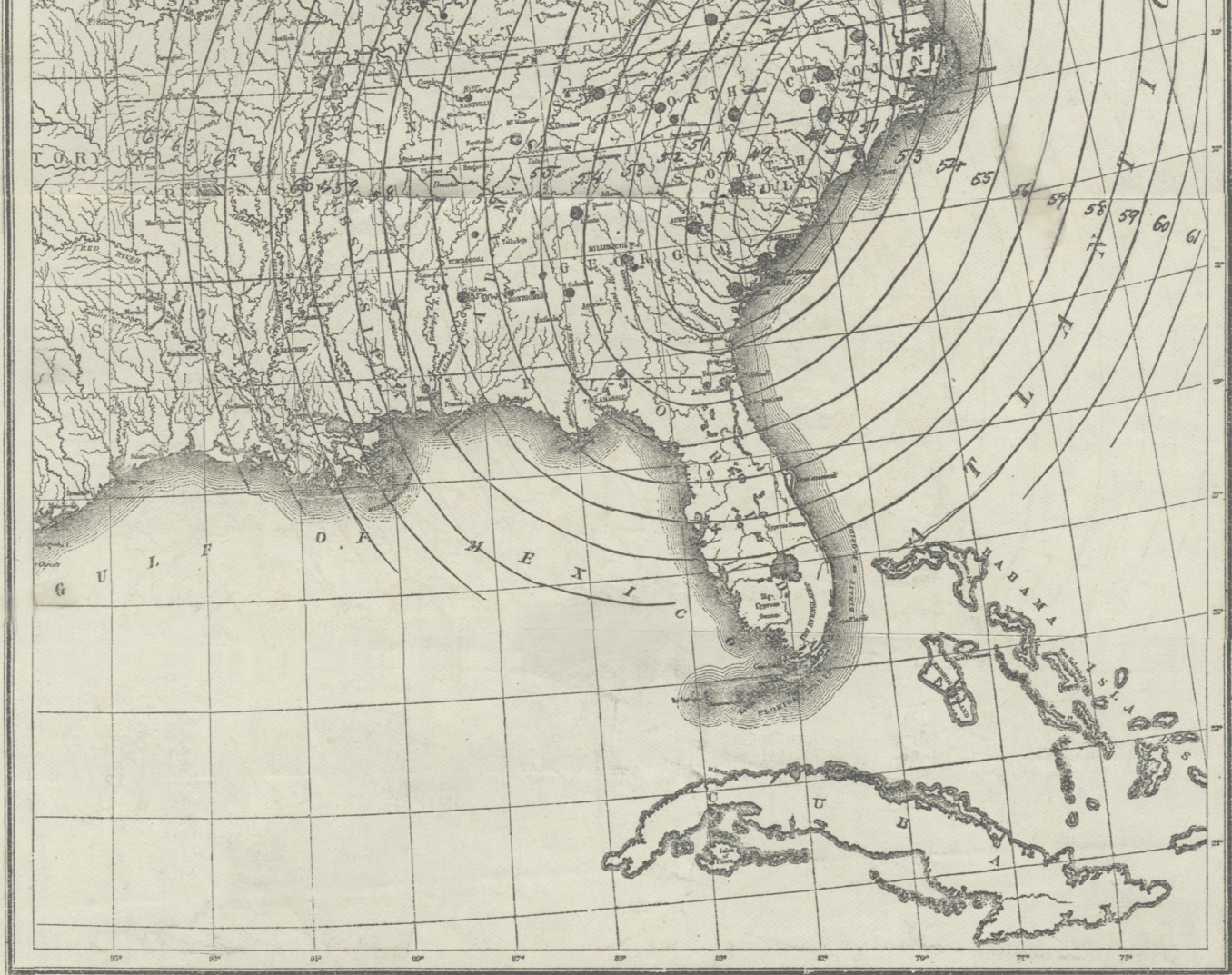
light. By the application of polarized light, 'amorphous crystals,' whatever these may be, may be detected. To determine whether these 'amorphous crystals' are of beef-fat or lard, the sample is boiled and slowly cooled, as already described, and mounted in oil. Under these conditions, he now finds, in accordance with Professor Weber, that butter, lard, and beef-fat all give globular crystalline bodies which (apparently with the exception of lard) show the Saint Andrew's cross. These bodies are to be distinguished by their forms, lard giving a stellar form, butter the well-known 'butter crystals,' and beef-fat a stellar form with biserrated spines. Dr. Taylor has also discovered the noteworthy fact that Tennessee butter of a certain grade yields globules which are flattened or indented on one side! The above account of Dr. Taylor's method, as at present described by him, is drawn mainly from his last annual report to the commissioner of agriculture, —his Chautauqua paper, to judge from the published abstract, having been chiefly a criticism on Professor Weber's experiments. We shall endeavor to keep our readers informed of the changes which the method undergoes in the future.

THE EARTHQUAKE OF AUG. 31, 1886.

THE accompanying map has been hastily compiled from the great mass of conflicting data from all sources now available, and probably gives a fair general idea of the origin of the shock, the limits of the area disturbed, and the intensity at many points within this area (plotted on the American scale of intensity, 1 to 5). It will be readily appreciated by every one that in this preliminary report all that is or can be arrived at is to give a general outline, as determined by the most probable evidence at hand, to serve as a good working hypothesis: to attempt any thing further at present would be to make a mere pretence at accuracy.

A line of weakness in the earth's crust extends from Troy, N.Y., south-westward, along the line of tidewater, past Baltimore, Washington, and Richmond, losing itself in a broad flexure south of Raleigh. The cause of the shock seems to have been a renewed faulting or displacement along the line where it crosses the Carolinas. This severe shock appears to have had its origin along this line in central North Carolina and eastern South Carolina, at 9.49 P.M. (75th meridian time), Aug. 31. It was not without warning. For a long time slight shocks have been occasionally felt in North Carolina, and only a few





THE CHARLESTON EARTHQUAKE.

The co-seismal lines give the even minutes after 9 P.M. (75th meridian time).

Scale of intensity, 1 to 5.

+ indicates that the shock was unimportant, or not felt.

Diameter of circles (in $\frac{1}{2}$ mm.) gives American scale of intensity (1 to 5).

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days previously moderate shocks were felt near Charleston. From the Carolinas it radiated with great rapidity (from 20 to 60 miles a minute) throughout the great area bounded on the south by the Gulf of Mexico; on the north by Michigan, the province of Ontario, New York, and southern New England; on the east by the Atlantic ocean, where it was probably felt nearly 500 miles at sea; and on the west by the central Mississippi valley. The limits are, so far as now known, as follows: central Florida; eastern Louisiana, Arkansas, Missouri, and Iowa; southern Michigan and province of Ontario; northern New York; and southern New England. It was not felt at Bermuda. The limits of the shock, as here stated and as indicated in the accompanying map, it is particularly desirable to verify, as well as the correct time at which the shock was first felt at all points within the disturbed area. It often happens that there are places within an earthquake area where the shock is not perceptible, owing probably to some local peculiarity in the geological formation, although decidedly noticeable at places not far away. There are already points of this kind mentioned,—in Florida, Indiana, and Connecticut, for instance,—and such information is very interesting.

The hypothesis has been advanced by Perrey that earthquakes are connected with subterranean tides due to the combined influence of the sun and moon, and analogous to those in the ocean. At a given point the earth's strata are under the accumulated tension of centuries, and this pressure is slowly but steadily increasing, until it reaches a point when fracture is imminent. Twice a day the great oceanic tidal waves sweep along the coast, the tremendous changes of pressure due to them being possibly augmented by analogous movements beneath the crust; and at a critical moment they add 'the last straw' that determines the fracture. It is very interesting to notice in this connection that at the time of the severe shock at Charleston this tidal influence was at its maximum. The moon was in perigee at 2 A.M., Aug. 29; new moon at 8 A.M. the same day, acting in a direct line with the sun (the eclipse of the sun occurred at 5 A.M., Aug. 29): extremely high tides occurred, therefore, for several days following. The moon's upper transit at Charleston occurred at 2.22 P.M., on Aug. 31. The high tide following (the higher of the two daily tides) was at 9.35 P.M., just twenty minutes before the shock occurred. This remarkable coincidence is of course extremely interesting.

It seems remarkable that no sea-wave followed the shock; and indeed it was providential that it did not, as the resulting destruction and loss of life

would have been a hundredfold greater. A sea-wave (often very incorrectly called a tidal wave) of greater or less size is the almost invariable accompaniment of a severe shock occurring near the seacoast.

It is unnecessary to enlarge here and now upon the general effects of this severe earthquake, or to theorize upon the causes of earthquakes in general or of this one in particular, more than has already been done. Such a study, to be of any value, must await the compilation and elaboration of a vast amount of material, and the final reports of the geologists who are now at work in the region of greatest disturbance.

STUDY OF THE EARTHQUAKE.

THE U. S. geological survey has undertaken to make a study of the severe earthquake of Aug. 31, which caused such great destruction and loss of life at Charleston, S.C. It was the most severe on record in the United States, both as to the effects produced and the area disturbed.

The study of phenomena of this kind is of the greatest value to science as a guide to the knowledge of the nature of the interior of the globe, and in its bearing upon every branch of physics and geology. In it there is needed a vast amount of reliable information, not only from points within the disturbed area, but also from adjacent points, in order to accurately define its limits; and it is not only skilled observers who can furnish such information, but almost every one can contribute valuable facts. It is therefore confidently hoped that facts of interest will be sent in at once to the U. S. geological survey at Washington while they are still fresh in the memory. Newspapers can render great assistance by giving wide publicity to this call, and by sending copies of their issues containing information about the local effects of the shock. Attention to the points mentioned below will add greatly to the value of the information, and facilitate its elaboration and study.

Write on one side only of the paper. After dating the letter as usual (giving also the locality where the observation was made, if not the same), write 'Answers to circular No. 2.' State the observer's situation (whether in the house or out of doors, up stairs or down, sitting, standing, walking, reading, etc.); also, if possible, the character of the ground (whether rocky, earthy, sandy, etc.) Then answer the following questions, referring to them by number only:—

1. Was an earthquake felt at your place the evening of Aug. 31, 1886, or within a few days of that time? Negative answers to this will be of great

interest from any points within the disturbed area, and especially from points near its limits; that is, southern Florida; central Mississippi, Arkansas, Missouri, and Iowa; south-eastern Minnesota and Wisconsin; central Michigan; southern portion of the province of Ontario; northern New York; southern Vermont and New Hampshire; and eastern Massachusetts; also from the western part of the Atlantic and northern part of the Gulf.

2. At what hour, minute, and second of standard time was it felt? When this can be accurately given, it is of the very greatest importance. Be particularly careful to state whether it is standard (railway) time or local time; whether the watch or clock was compared with some standard clock at a railway-station or elsewhere, how soon, what the error was, and whether you corrected your observation by this comparison or not.

3. How long did its perceptible motion continue?

4. Was it accompanied by any unusual noise? If so, describe it.

5. Was there more than one shock felt? If so, how many? Where several were felt, give accurately, or even roughly, the number, duration, and character of each, and the interval between them.

6. Which of the following measures of intensity would best describe what happened in your vicinity?—No. 1. Very light; noticed by a few persons; not generally felt. No. 2. Light; felt by the majority of persons; rattling of windows and crockery. No. 3. Moderate; sufficient to set suspended objects, chandeliers, etc., swinging, or to overthrow light objects. No. 4. Strong; sufficient to crack the plaster in houses or to throw down some bricks from chimneys. No. 5. Severe; overthrowing chimneys, and injuring the walls of houses.

7. Do you know of any other cause for what happened than an earthquake? Give also any further particulars of interest, stating whether they are from observation or hearsay: for instance, whether the shock seemed like a tremor or jar, or an undulatory movement; and whether it seemed to come horizontally or vertically; whether any idea of direction of shock was formed, and if people agreed in their idea as to such direction. Mention any unusual condition of the atmosphere; any strange effects on animals (it is often said that they will feel the first tremors of a shock some time before people notice it at all); character of damage to buildings; general direction in which walls, chimneys, etc., were overthrown. Springs, rivers, and wells are often noticeably affected by even slight shocks, and such facts are especially interesting. If a clock was stopped, give the time it

indicated, and some idea as to how fast or how slow it was, its position, the direction in which it was standing or facing, and the approximate weight and length of the pendulum. If a chandelier was noticed to swing decidedly, describe it and state direction of swing. If pictures swung, state direction of wall, and whether pictures on the wall at right angles to it were also put in motion. If doors were closed or opened, state the direction of the wall in which they were set. All such little facts, if only noticed, remembered, and recorded, are of great value.

At end of letter give name of the observer, if other than the writer. A moment's thought will show the impossibility of an immediate acknowledgment of every letter received, although each one will have its share in contributing to the value of the result, as it finally appears in the public press and the official publications of the survey.

Address simply, Division of volcanic geology, U. S. geological survey, Washington, D.C.

EVERETT HAYDEN, *Assistant Geologist.*

THE FRENCH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

THE French association for the advancement of science held its fifteenth annual meeting in Nancy, the 12th of August and the week following. Nancy, one of the frontier towns, near the German limit, is a very handsome and pleasant city. It is very prettily built, and contains old monuments of a striking effect. It is also a scientific and literary town, and many able *savants* or writers hold a position in the university. The meeting was a very successful one, in that a large number of members were present, and the papers submitted were numerous and satisfactory. The president was M. Friedel, the well-known chemist, the successor of Würtz in the Sorbonne, and one of his best and most affectionate pupils. In his address to the meeting the first day, he made it known that the Association scientifique, founded by Leverrier, is to be soon combined with the French association under the name of the latter. The greater part of M. Friedel's address was concerning recent progress in chemistry and mineralogy. After having recalled M. Moissan's successful experiment, by which fluor has been isolated for the first time, and M. Lecoq de Boisbaudran's interesting researches concerning two new metals, he spoke at length on the artificial synthesis of different compounds, such as those of feldspars and some precious stones. After M. Friedel's address, M. Collignon, the secretary-general, briefly recalled the principal points of the association's history for the past

year, alluding to the Grenoble meeting and the excursions made in the neighborhood, mentioning the names of deceased members: Bouquet, Bonley, Jamin, Robin, Dechambre, Courty, and others. M. A. Volland, mayor of Nancy, greeted the association with heartfelt words. M. E. Galante spoke on the financial state of the association, which is very satisfactory. The expenses are for the publication of the yearly volume recording the acts of the association and the different works submitted; many grants for scientific researches are also included.

Some interesting discussions have been held in the meetings of the different sections. One of the best took place in the agricultural section, and the topic was wheat-production. Many experimenters and able specialists took part in this discussion, such as Frederic Passy, Levasseur, Alglave, Dehé- rain, Grandeau, Raffalovich, etc. M. Dehé- rain spoke on the best manner of getting the most wheat at least cost price, which is, I think, the universal desideratum, applying not only to wheat, but to all that can be manufactured or grown. M. Dehé- rain said that the great objection to the use of a large amount of manure is the 'laying' which usually occurs. But the 'laying' can very well be avoided if some trouble is taken in selecting the wheat species. According to M. Dehé- rain's experiments, the Scotch red wheat, the Shirley, and the Browick are not subject to 'laying,' and the crop is a very fine one when manure is liberally used; 35 or 40 *quintaux* of wheat, and 60 or 80 of straw, sometimes bringing more than 500 francs per hectare. M. Porion has even been able, in the Pas-de-Calais, to obtain crops four times more abundant than the mean average of French crops. M. Sagnier spoke of Indian wheat, the hero of the day, but a very unwelcome one. It seems that India is growing wheat very successfully, and the increased extension of railways helps this production in a marked manner. In 1876, ten years ago, India had twelve thousand kilometres of railway, and one and a half million hectares planted with wheat. At present there are thirty thousand kilometres of the former, and twelve million hectares of the latter. In ten years the wheat-crop has increased eightfold: it has doubled in the last three years. But this cannot be all, and the wheat-crop must certainly become greater still. M. Sagnier believes it may certainly become double what it is at present, and four times as large as that of France at this time. M. Alglave agrees with M. Sagnier, because, he says, although the inhabitants of North India have taken to using wheat for their food, those of the south keep eating rice, which does not sell so easily; and all their wheat they willingly sell, in-

asmuch as rice-culture does not interfere with wheat. Rice requires a watery soil, which does not suit wheat; so that they will continue growing rice in the valleys, and wheat on the hillsides. At all events, the enormous extension of wheat-culture in India is a matter of no little anxiety to European agriculturists.

In the anthropological section, M. Cartailhac read a paper concerning primitive burial rites. In 1830 some Danish anthropologists, Bruzelius, Boye, and Hildebrand, believed that in many cases primitive men were accustomed to bury only the bones, after the flesh had disappeared. M. Cartailhac, following up this idea, remarked that in many savage countries the fact is quite true. In the Andaman Islands, for instance, as E. H. Man has recently noticed, the body is buried for a time only, then unearthed when the flesh has been decomposed; and a similar custom is met with in many instances. M. Cartailhac proves that this fact is also established in regard to primitive mankind, and that at the *age du Reune* — nothing being known of the burial rites of the stone age — the real burial was performed only when the body was deprived of flesh. In the Menton caves, for instance, the bodies were certainly buried in the skeleton state. The same is true of the *age de la pierre polie*. Upon the whole, M. Cartailhac believes that the custom of letting corpses putrefy before giving them a definitive burial has been a very prevalent one. It is curious enough to notice that in Spain no king is laid in his burial-ground before the death of his successor: the dead king remains in the *Putrido*, as it is called, till his successor comes to take his place.

In the medical section I notice no very interesting papers yet, that is, none of general interest. There have been no general meetings at this session, as there usually are, — none save the general assembly of the first day. Some interesting excursions have been made in the neighborhood. One had been projected to Mount Douon, a mountain on the German territory; but the German authorities, not knowing the nature of the French association, had asked that no excursion should be made: so it was deemed better to abandon the project. Only two or three persons went up, and found a small body of troops and some local German authorities. But it was ascertained that the intentions of the association had been entirely misunderstood, the German authorities knowing nothing of the association, and believing it to have political objects. The absurdity of the notion was ridiculed, and no more was thought about it.

The next meeting will take place in Toulouse, and the following one, for 1888, in Oran (Algeria).

LONDON LETTER.

ANOTHER of the veteran English naturalists has just passed away, after a long illness, in the person of Mr. George Busk, F.R.S., F.G.S., etc. By profession a medical man, and for many years consulting surgeon to the Seamen's hospital at Greenwich, he was one of those who sacrificed his professional prospects to a love of science for its own sake, and made his reputation chiefly as a teacher and examiner in the subjects of comparative anatomy and histology, in connection with the Royal college of surgeons and the University of London. He was one of the translators and editors of Kölliker's 'Manual of human histology,' and sole editor of Wedl's 'Rudiments of pathological histology.' In 1872-73 he was vice-president of the Royal society, and for about ten years was the secretary of the Linnean society. When an inspector of physiological laboratories was needed under the vivisection act, Mr. George Busk was appointed to the post, which he held, with great advantage to science, up to the time of his death.

A severe colliery explosion has just taken place in one of the deep pits (1,410 feet) of the Lancashire coal-fields, by which nearly forty lives were sacrificed; but one of the few survivors is able to give most important evidence on the behavior of the Davy lamp, and its share in causing this particular explosion. From the heaving of the coal, a sudden rush of gas came out upon his partner's lamp, the flame rapidly elongated inside, and in a very short time the gauze was seen to burst, and the explosion took place. This danger was not unknown to Davy, but it has hitherto been considered that the elongation of the flame gave sufficient warning to enable the miner to escape to a place of safety. In the present instance it seems clear that the three stages followed each other too quickly, the result being a lamentable sacrifice of life.

Considerable attention has been directed of late to the performances of the Marchant engine, for which it is claimed that the difficult problem of the return to the boiler of steam which would otherwise be wasted has now been practically solved by it. Several stringent tests have been made of this engine under the superintendence of responsible engineers previously unacquainted with it, and the result of one of the most recent may be here given. "The stated effective horse-power of the engine (93.3) was therefore obtained at the expenditure of 0.803 pounds Welsh coal per horse-power per hour, and we hereby certify to such ascertained result." The boiler pressure was 241 pounds per square inch; the average vacuum in the condenser, 17 inches; and the speed, 125

revolutions per minute. The results thus obtained work out to a fraction over half a pound per indicated horse-power per hour. The economy in coal is such, that it is calculated that the Peninsular and oriental steamship company would save £1,000 (\$5,000) per day by the use of such engines. As the condenser occupies only a sixteenth of the space of an ordinary water-condenser, it is adaptable to locomotives, which might, says Mr. Marchant, the inventor, be built to run 1,000 miles without a fresh supply of water.

The season of annual congresses has now well begun. Allusion was recently made in this correspondence to that of the naval architects in Liverpool, a concise summary of whose work appears in *Nature* for Aug. 12, and will well repay careful perusal. The mechanical engineers hold theirs in London during the present week. The controversy upon women's education, revived by Dr. Withers Moore in his presidential address at the British medical association last week, has already received contributions by cable from the United States, and has attracted much attention here. The educated layman's view of it is forcibly set forth in an article headed 'A plea for silly mothers' in the *Pall Mall gazette*, from which the following sentences may be quoted. "Where Dr. Moore has gone astray is, that while he wants to prove that the higher education unfits women to be mothers, all he does is, that overpressure does so: of course it does. Overpressure is bad for women; so it is for men. Some women are not fit for professional careers; neither are some men. . . . We no longer aspire to shut women out of the world in mediaeval seclusion; our aim is rather to keep them among its stir and strife in protected and shepherded peace, and in that work there is as much call upon the new chivalry as ever was made in an earlier civilization upon the knights of the lance and spear."

Dr. J. S. Billings's address on the position and prospects of the medical profession in America excited very great interest, as did the invitations from the American representatives to attend the International medical congress to be held next year in Washington. The present meeting has been more cosmopolitan than any former one, a hundred members coming from the continent, United States, or colonies, while there were members from Costa Rica, Calcutta, Japan, and South Africa.

A very interesting discussion, which has a scientific side to it, is going on with reference to the permanency of water-color pictures; and so much public interest has been aroused, that a committee has been appointed by the 'lords of the committee of council on education' to investigate

and report on the matter. The advocates of the permanency of water as a medium for color-painting cite in support of their views the fact that the ancient Egyptians, whose pictures in some cases are apparently as fresh and bright to-day as when first executed two or three thousand years ago, used water-colors. Old manuscripts, illuminated in water-colors several centuries ago, do not appear to have diminished in brilliancy. On the other hand, there are undoubted cases of fading of pictures by Turner and others, owing to prolonged exposure to sunlight. A comparison of collections of oil and water color pictures of equal age, however, seems to show that the former are at least as liable to fade as the latter. Such colors as ochres and siennas are permanent in both mediums, while lakes are fugitive in both, and the madder colors are generally considered lasting. A few years ago, Mr. Holman Hunt took much trouble to investigate the purity of artists' colors, which, he found, were frequently adulterated. The results he communicated at the time to the Society of arts. It is now suggested that a more extended official investigation should be made of the whole subject, on the lines which he then indicated, including in the research the action of the electric light, as well as that of sunlight, upon pure and adulterated pigments, and mixtures thereof.

W.

London, Aug. 15.

NOTES AND NEWS.

THE two hundred and fiftieth anniversary of the foundation of Harvard university will be celebrated on the sixth, seventh, and eighth days of November next. On Saturday, the 6th, undergraduates day, the students of the university will celebrate the event by literary exercises in the morning, athletic sports in the afternoon, and a torchlight procession in the evening. On Sunday, the 7th, foundation day, the anniversary of the passage by the general court of the colony of Massachusetts Bay, of the memorable vote, "The court agree to give four hundred pounds towards a school or college, whereof two hundred pounds shall be paid the next year, and two hundred pounds when the work is finished, and the next court to appoint where and what building," there will be commemorative exercises, under the direction of the college authorities, in Appleton chapel, conducted in the morning by Rev. Francis G. Peabody, and in the evening by the Rev. Phillips Brooks. On this day clerical graduates of the university are requested to refer in their pulpits, if the circumstances permit, to this act of the infant colony, and the benefits which have followed from

it. On Monday, Nov. 8, alumni day, the graduates of all departments of the university, and guests, will meet in Massachusetts hall, at 10 A.M., and proceed thence to Sanders' theatre, under escort of the undergraduates, where an address will be made by James Russell Lowell, and a poem delivered by Oliver Wendell Holmes, and honorary degrees will be conferred by the university. In the afternoon the association of the alumni, composed of all graduates of the college, with their invited guests, will have a collation in Memorial hall. It is suggested that the members of Harvard clubs in the various cities of the United States who are unable to attend the celebration at Cambridge should commemorate the day.

—The American public health association will convene at Toronto, Canada, Tuesday, Oct. 5, and continue four days. The executive committee have selected the following topics for consideration at said meeting: the disposal of the refuse matters of cities and towns; the condition of stored water-supplies, and their relation to the public health; the best methods and the apparatus necessary for the teaching of hygiene in the public schools, as well as the means for securing uniformity in such instruction; recent sanitary experiences in connection with the exclusion and suppression of epidemic disease; the sanitary conditions and necessities of school-houses and school-life; the preventable causes of disease, injury, and death in American manufactories and workshops, and the best means and appliances for preventing and avoiding them; plans for dwelling-houses. At the last annual meeting of the association, a resolution creating a section of state boards of health was adopted. A meeting of the representatives of the state boards of health has been called by the secretary of the Conference of state boards of health, on Monday, Oct. 4, and it is expected that the said representatives will on that day organize the section.

—The hydrographic office has received the following note: Aug. 31, at 9.45 P.M., the steamer City of Palatka, Captain Voegel, when a mile and a half north of Martin's industry light ship (off the coast, south of Charleston), in eight and a half fathoms of water, experienced a terrible rumbling sensation, lasting a minute and a half. There was quite a heavy sea from the south-east after leaving Charleston bar at 5.30 P.M. When this rumbling sensation took place, the wave-motion ceased. It was a perfect calm during the rumbling: after that, the usual motion of the south-east swell took place. The wind at the time was south-west, light, weather cloudy, barometer 30° 01', thermometer 80°. The sensation resembled a

ship scraping a pebbly bottom, and the vibration of the ship was very great.

—A very interesting account of an epidemic of malaria in eastern Massachusetts is given by Dr. L. B. Adams in the *Boston medical and surgical journal*. This epidemic of intermittent fever occurred in the summer of 1885, and its chief force was felt at South Framingham. The infected district contains one-third of the area and one-fifth of the population of the village. Five-sevenths of the houses had cases in them. In some instances every occupant was attacked. A few scattered cases were seen in June and July. At the close of July there was a change of weather and a heavy fall of rain. This was immediately followed by the appearance of many cases. August was colder than it had been for fifteen years, and the rainfall great, more than seven inches. Between the end of July and the latter part of September, when the disease began to decline, more than two hundred cases were seen and reported by the physicians. It was thought by some that the disease was attributable to the drinking-water. The full history of this epidemic, and the views of Dr. Adams, will doubtless be given in the future, and we shall then refer to this subject again.

—Special attention should be paid by bathers to the exclusion of salt water from the mouth and ears. Many cases of inflammation of the ear, followed by severe and lasting trouble, even to deafness, are chargeable to the neglect of this precaution. Incoming waves should never be received in the face or the ears, and the sea-water which enters the ears when floating or diving should be wiped out by soft cotton: indeed, the best plan is to plug the openings of the ears with cotton, which is to be kept there during the bath.

—The new State board of health of Massachusetts is composed of seven gentlemen, two of whom are physicians,—one a regular, Dr. H. P. Walcott, who is president of the board; and the other, Dr. E. U. Jones, a homoeopathist. Dr. S. W. Abbott, a well-known sanitarian, has been appointed secretary.

—Profs. von Frisch and Ullman of Vienna, after careful and exhaustive study, confirm the views of Pasteur as to the possibility of preventing the development of rabies by inoculations with the virus obtained from rabbits, and are now prepared to treat the victims of rabid dogs.

—Sea-bathing is now so generally practised, and death by drowning so common, that every person should familiarize himself with some method of resuscitation; and if each community living upon the seashore or upon the banks of

rivers or bays would organize a life-saving service, or obtain instruction in this important subject, many lives which are now sacrificed would undoubtedly be saved. One of the simplest methods of artificial respiration is that which Mr. J. A. Francis has described in the *British medical journal*. The body of the patient is laid on the back, with clothes loosened, and the mouth and nose wiped; two bystanders pass their right hands under the body at the level of the waist, and grasp each other's hand, then raise the body until the tips of the fingers and the toes of the subject alone touch the ground; count fifteen rapidly; then lower the body flat to the ground, and press the elbows to the side hard; count fifteen again; then raise the body again for the same length of time; and so on, alternately raising and lowering. The head, arms, and legs are to be allowed to dangle down freely when the body is raised.

—Two more persons inoculated by Pasteur for the prevention of rabies, after having been bitten by rabid dogs, have died. Of fifty-four persons bitten by mad wolves, fourteen have died.

—One of the leading men of Edgefield county, S.C., is reported to have died this week from rabies, after an illness of but twelve hours. The bite was received in May from a rabid dog, and produced no trouble until the day before his death.

—In a little pamphlet, under date of Aug. 22, the Hon. W. E. Gladstone has given his views on 'The Irish question' as it now stands, with a history of the movement for self-government and an indication of the lessons taught by the recent election. This has been published on this side by Charles Scribner's Sons, New York.

—'A catalogue of minerals,' by Albert H. Chester (New York, Wiley, 1886), is intended to embrace all English names now in use in the nomenclature of mineralogy. It includes species, varieties, and synonyms. Dead and useless names have been omitted, so that the catalogue can be conveniently used as a check-list and in cataloguing collections.

—The weather report for August, from observations taken at Lawrence, Kan., shows that the month was one of the three hottest Augusts on record. There were eleven days with temperature below the average for the season, but the remaining twenty days were excessively hot; and the week from the 11th to the 17th surpassed any week since August, 1874. The July drought was broken on the 1st by a copious shower. There were seven other serviceable rains during the month, but no rain sufficiently heavy to wet the ground to a greater depth than two inches.

— Glanders is said to be quite prevalent among horses at the present time. The New York state board of health has discovered six cases at Middletown.

— The Paris *Conseil municipal* has ceded to the Society of the *Institut Pasteur* for ninety-nine years the ground upon which the institute is built. The following official statement has just been made: The whole number of persons treated by Pasteur is 1,656 (of these, 15 have died); 1,009 of these were French (3 of them died); 182, including 50 bitten by rabid wolves, were Russians (3 of these bitten by dogs, and 8 by wolves, have died); 20 were from Roumania, with one death; of the others, 59 were from England, 17 from Austria, 74 from Algeria, 18 from America, 2 from Brazil, 42 from Belgium, 58 from Spain, 7 from Greece, 8 from Holland, 25 from Hungary, 105 from Italy, 20 from Portugal, 2 from Turkey, and 2 from Switzerland (of all these, not one has as yet died: the total mortality, therefore, is less than one per cent, — a most striking commentary upon the views of those who declare Pasteur's methods a failure).

— At the last meeting of the American association, Eugene Michel Chevreul, on the motion of the section of chemistry, was elected an honorary fellow, the second only on the rolls of the association.

LETTERS TO THE EDITOR.

*.*Correspondents are requested to be as brief as possible. The writer's name is in all cases required as proof of good faith.

Dynamite explosions.

IN its issue of the first inst., referring to the recent Chicago explosion, the New York *Herald* publishes, under the heading 'Teachings of the explosion,' an article containing 'some things' claimed to be 'instructive and important,' but which are so incorrect as to be neither. In this article it says, "But we know now, happily at the cost so far of but two human lives, some things that are instructive and important. One is, that a huge mass of dynamite, say ten tons, even when blended with five times its weight of gunpowder, expends its main force downward, thus verifying, on a vast scale, a fact known of the explosion of much smaller quantities of dynamite. Another fact is, that dynamite, even in huge volumes, is less likely to ignite neighboring masses of explosives in such a casualty than an unmixed mass of gunpowder would be. There were ten large magazines close to the Laffin & Rand, and all escaped ignition."

The above statement, that 'a huge mass of dynamite,' in exploding, 'expends its main force downward,' and the deduction that this verifies, "on a vast scale, a fact known of the explosions of much smaller quantities of dynamite," are so scientifically inaccurate as to need correction. The fact is, dynamite explodes with equal force in all directions, and that, at whatever point it meets with the greatest

resistance, at that point it is most destructive, whether it is upward, downward, or laterally.

It is a common error, however, that dynamite always 'expends its main force downward,' which arises, probably, from the fact that, in the majority of reported dynamite explosions, it has met with the greatest resistance from the earth, and therefore has exhibited its 'main force' in that direction.

Mr. G. M. Roberts, manager of the Nobel's explosives company, London, writes as follows to the *London Times*: "Nitroglycerine and dynamite do not, when exploded, exert such a force as is popularly believed. To speak precisely, the power developed by the explosion of a ton of dynamite is equal to 45,675 tons raised one foot, or 45,675 foot tons. One ton of nitroglycerine similarly exploded will exert a power of 64,452 foot tons; and one ton of blasting gelatine, similarly exploded, 71,050 foot tons. These figures, although large, are not enormous, and need not excite terror. Seventy-one thousand tons of ordinary building-stone, if arranged in the form of a cube, would measure only 90 feet on the side, and, if it were possible to concentrate the whole force of a ton of blasting gelatine at the moment of explosion on such a mass, the only effect would be to lift it to the height of a foot. The foregoing figures are derived from experiments made at Ardeer with an instrument which gives accurate results in measuring the force of explosives."

Supposing these data to be reliable, and in view of the fact that the buildings which stood on the great excavation in Chicago have disappeared entirely, is it not reasonable to suppose that fully as much force was required to lift, splinter, and distribute, in every direction, the materials composing those buildings, overcoming the attraction of gravitation in the act, as was necessary to make the great excavation in the earth, by the expenditure of 'its main force downward'?

This fact of the elimination of the buildings seems to have escaped the notice of the writer of this article.

In verification of our statement that it explodes with equal force in all directions, the following extract from the above quoted authority, Mr. Roberts, is cited: "I have often, by way of experiment, exploded a pound of dynamite suspended from the end of a fishing-rod by a string about six feet long, holding the rod in my hand the while. As there was no solid matter to project, I received no injury, and the end of the fishing-rod was not even scratched. About three feet of the string at the end of the rod was always left uninjured."

Meeting, in the above experiment, with no resistance other than the air at any point, there was consequently no destructive power shown in any direction; but, had there been solid matter above or below or on either side, the 'main force' would have been expended upward or sideways, and not 'downward.'

This experiment illustrates another remarkable feature in dynamite, peculiar to itself, — that of its concentrated or local effects, compared with the more diffused effects of gunpowder explosions.

Quoting again from Mr. Roberts, he says, "The power exerted by an explosion on surrounding objects is in the inverse ratio of the cube of the distance from the point of explosion. Thus, at 100 feet from the exact point of an explosion, the power is only the cube of 1-100 or 1-1,000,000 part of what it is at a distance of only one foot from that point, or, in other words, if the power at one foot from the spot be represented

by 1,000,000, at the distance of 100 feet it will be but 1. It is thus seen that the effects are intensely local, and but comparatively trifling at even short distance."

The wide-spread damage in the Chicago explosion was undoubtedly due, in a much larger degree, to the gunpowder than to the dynamite exploded.

Another fact and deduction relating to the escape of several magazines near the great explosion are quite as misleading, if not as erroneous, as the former ones.

If we are correctly informed, most, if not all, of the magazines nearest the exploded buildings, contained dynamite. Now, it is a fact well known to experts that this material is non-explosive by shock or by fire applied separately, but requires some fulminate combining both concussion and combustion, acting simultaneously, to explode it. Hence, being protected from the fire or combustion of the explosion by the walls of the magazines, and being unsusceptible to the force of the concussion, there is nothing remarkable in the salvation of the adjacent magazines. Even those, if any, which contained gunpowder (that explosive being protected from contact with fire, and remaining inactive) were uninjured for equally scientific reasons.

The article concludes, referring to its statements and deductions, by saying, "These are facts which could not have been exemplified save at much cost and risk, and our government officers and other men of science will, we may be sure, bear them carefully in mind hereafter."

Now, as we have shown that the above statements are not facts, but that the contrary is the real truth, and as the actual facts have been ascertained as well by many of our government officers as by a large number of experts all over the world, we would respectfully suggest to the *Herald*, when it intends to publish another scientific dynamite article, that it secure the services of, or at least submit its facts to, some such expert as General Abbott or Gen. John Newton, both of the U. S. army, whose experience with explosives of every kind has been exhaustive, and thus obtain information that the public can rely on.

A. W. G.

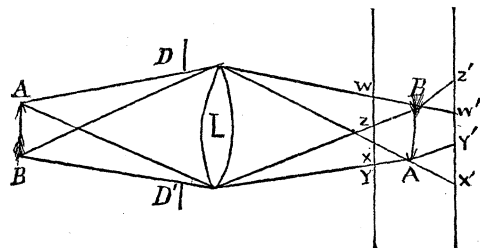
New York, Sept. 1.

On a means of determining the limits of distinct vision.

If an image ($A'B'$) of an object (AB) be thrown on a screen by means of a lens (L , for simplicity supposed free from spherical aberration), and the screen moved forward or backward, the image will be blurred. If part of the rays be stopped by a diaphragm (DD'), this blurring will be less as the aperture of DD' diminishes, for this lessens the spaces (WZ , etc.) over which the rays from any one point of the object are spread on the screen. Now, let the rays be cut off from one side alone; let a curtain (D) descend from above. The upper boundaries (W, X , etc.) of the spaces WZ, XY , etc., will descend, while the lower ones remain stationary. If the object be dark against a brilliant background, the light from above A will be cut off as B descends, and the blurred edge (XY) of the image becomes dark; so that, in the limit, instead of a blurred image (WY), there would be a distinct one (ZY), or, as the image of D ascends, the image of AB appears to move to meet it, the part near D leading the way, since D intercepts the extreme ray from A before that from B .

If the object be light on a dark ground, the effect will be most apparent on the boundary farthest from D , since the blurred edge that changes to dark is more noticeable than that which changes to light. If the image be formed in front of the screen, making the blurred image $Z'X'$, a little consideration will show that the apparent motion of the image will always be away from the image of D .

These results may be verified with any lens, but are most strikingly shown with the eye, using a sheet



of paper close to the eye as curtain, and any object,—as a pin, pencil, or ruler,—seen against a window or lamp as background. A slit in a piece of paper held against a lamp serves as light object on a dark ground. It is, of course, easy to hold the object so near that it will be blurred; but special effort may be required to blur a distant object, except with near-sighted persons. The applicability of this in making a test of the limits of distinct vision is now apparent. Let a ruler lean against the shade of a lamp; place the eye so near that the image is necessarily blurred, and, moving the edge of a sheet of paper back and forth before the eye, step slowly backward till apparent motion of the object ceases; continue the backward movement until the object begins to recede slightly from the screen: the space where there was no motion is that in which alone distinct vision is possible. Of course, every effort must be made to accommodate the focus of the eye to the object during the whole experiment.

It is a more difficult task than one thinks, to decide by simple judgment whether an object is seen distinctly or not, except it be much blurred. If the image is fairly distinct, most people will suppose it to be perfectly so. The test described above never fails to show whether or not the judgment is correct.

The effect noticed above also adds to the appearance seen when two networks of thread or wire not in the same plane are held before the eye. The watered appearance is of course due to curves which are the loci of the intersections of one set of wires with the other; but these intersections are made noticeable by the fact, that, when two wires not in the same plane and making an acute angle are held before the eye, the nearer acts the part of the curtain D in the above demonstration, and an irregular dark spot is seen about the point where the wires cross. The writer hopes to make a series of experiments as to the limits of distinct vision in different persons, using the test suggested above. Its simplicity, and the absence of any judgment on the part of the person experimented upon, other than as to the direction of motion of the object, commends it to the investigator.

ARTHUR E. BOSTWICK.

Montclair, N. J., Aug. 30.

Cause of a recent period of cool weather in New England.

From Aug. 15 to Aug. 23 the weather in New England was quite cool and pleasant. This cool period culminated on the night of the 22d, when the temperature at the Boston signal office sank as low as 49°. On the signal service weather-chart of the morning of Aug. 23, it is found that the temperature was higher all around New England (north, east, south, and west) than in New England itself. Over New England the sky was clear, and the air was blowing out from this region in every direction, on the east side toward a storm which is central on the ocean, and on the west side toward a storm which is central in the lake region. Whence, then, came this cool air? for it had previously been quite warm. It evidently could not have been imported from abroad: was it, then, due to a descent of cool air from above? This is hardly possible, since it was found, at 11 P.M. of the 22d, that the temperature on Mount Washington was 51°, while at the nearest lower stations—Portland and Boston—the temperature was 56°, and on top of Blue Hill 51°. At 7 A.M. of the 23d the conditions of temperature were almost the same, except that the temperature had risen slightly at every station but Boston. If the air had descended from the height of Mount Washington, it is well known that its compression would have heated it much higher than the temperature was found to be at lower stations, unless this heating had been counteracted by some other cause. On top of Blue Hill the lowest temperature recorded by a self-registering minimum thermometer on the night of Aug. 22 was only 50.5°; while, at a base station four hundred feet lower, the temperature fell to 44°; and in Boston, nearly six hundred feet lower and ten miles distant, the temperature fell to 49°. The thermometers were alike, and exposed in the same manner. The air evidently descended over New England from above, otherwise the wind could not have blown out in every direction; but the statistics above show that its coolness could not have been due to this cause, since it was cooler at the earth's surface than a little distance above it. The air, as was to be expected on account of its descent from above, was clear and dry, the absolute humidity being lower than at any time during the month except on the night of Aug. 15, when almost identical conditions prevailed. Here we no doubt find the cause of the coolness. Tyndall's experiments on the effect of aqueous vapor in intercepting radiation from bodies of low temperature like the earth led him to assert, that, if the blanket of aqueous vapor over England were removed for one summer's night, the whole island would by morning be held in the iron grip of frost, on account of the rapid radiation from the earth's surface which such conditions would permit. Even the more intense insolation by day at such time would be counteracted by the rapid radiation into space, as shown at elevated parts of the earth's surface. This serves to explain the cool period lasting several days in New England; and this cool period seems to substantiate the view recently advanced, that the cold in anticyclones (or areas of high pressure) is due to radiation from the earth's surface, which is favored by the clear, dry atmosphere accompanying these areas. Tyndall, Hann, and Woeikof have adduced evidence of this in Europe, and Mr. Dewey in this country (see *Amer. met. Journ.*, May, 1886).

H. HELM CLAYTON.

Blue Hill meteor. observ., Aug. 30.

Dr. Orton's Ohio gas and oil report.

I have been carefully studying my friend Dr. Orton's admirable and most valuable report on the Findlay, Bowling Green, and Lima wells, an advanced summary of which you published in the issue of *Science* for June 25. Having been absent from my office, I am ignorant as to whether your subsequent issues contain notices or criticisms of Dr. Orton's facts and views, which I esteem not only historical, but marking an era in our knowledge of the subject. I run some risk, therefore, of offering considerations which others may have anticipated; but two or three of these considerations deserve attention in the present stage of our investigations.

I trust that all geologists will sympathize with me in heartily cheering Dr. Orton's skilful insertion of the long-awaited for keystone in the arch of the demonstration of the origin of oil. I am ashamed of my own stupidity in not finding and fixing in its place this keystone myself. I have been seeking it for years, asking myself continually how the decomposing organic matter of the seashores and marshes could be retained by the sands and shales until sufficiently protected from complete oxidation. I have repeatedly put this question to other geologists, but never received an answer of any kind; apparently because so few of them accepted the *in situ* origin of rock-oil, and therefore seeing no value in the question, and no need for an answer to it. Dr. Orton is the first geologist to appreciate the value of Dr. Leidy's observation of the petroleum-mud-layer at the mouth of the Schuylkill River; and his generalization from it is one of the best and broadest ever made in our branch of science. It accounts satisfactorily for the preservation of rock-oils in every formation, of every geological age, all over the world; subject, however, locally or regionally, to subsequent change or destruction. The eruptive rocks (lavas proper) are the only formations not charged with organic matter. Even the tufas, swept by the wind into the sea, must hold the remains of animal air-life and plant pollen. The winds are forever transferring dead and living organisms from place to place, and every rain washes them to the surface of the land and sea to be locked up in clay formations. However different the regional conditions, the process is continual and the results identical everywhere. Compare the Levant with the Red Sea. Each is as large as our Appalachian belt from Canada to the Gulf of Mexico. The one, however, is a reservoir of Nile deposits,—an extension of the Delta under sea-level,—replete with the original stuff of rock-oil. The other is a reservoir of incalculable quantities of wind-deposits, mixed with equally incalculable quantities of tropical animal and vegetable organic stuff. If any one still doubts the *in situ* theory, let him try to invent any other for the vast expanse of petroleum ground on both sides of the Caspian, and of course including the bed of that sea. There, also, we see going on at present the slow process of the loss of rock-oil from a formation which was originally charged with it; and that without any great structural disturbance. In Galicia, in Lombardy, on the other hand, we see the process of loss nearly finished under conditions of structural disturbance so great as to make the dips vertical. If Oken had been a geologist, and were living now, he would probably assert in his next treatise—and with a certain magnificent truthfulness—that the whole crust of the globe consists only of oiled clay, whether siliceous, ferruginous,

or calcareous,—here, in its original condition; there, oxidized and dried; in another place, crystallized and cleansed. Fortunately the Okens are not all dead; but their generalizations are restrained by a wise caution as to the genuineness of facts, and regulated by measurements. Dr. Orton's report proves this in a most satisfactory manner—if it needed proof.

The difference between the Pennsylvania and north-west Ohio oil and gas regions is fourfold: 1°, one is Devonian, the other Silurian; 2°, one is sandstone, the other limestone; 3°, one is decidedly waved, the other almost on a dead level; 4° (and this is what I wish specially to discuss), the one is non-cavernous, the other cavernous.

Thirty years ago I began to insist upon the geological (especially the topographical) importance of the underground chemical and mechanical erosion of the limestone formations of the world. I was led to this by my first field-work in the Silurian valleys of Pennsylvania and my early study of the blue-grass country of Kentucky. I saw that the eastern and western coal-fields had been separated by the falling-in of the roofs of myriads of mammoth caves in the Trenton limestones, preceded by the same process at the outcrops of the cavernous subcarboniferous limestones. I have always opposed the notion of the early age of the Cincinnati uplift. The nonconformity in middle Ohio, and that around Nashville, are important facts, but merely mark two out of many local and temporary variations in the general downward movement, which was otherwise uninterrupted from Silurian times to the end of the coal-measure age. It was not until then that the great upward and plicating movement took place, which started the erosion of the United States area. The principal rôle in the erosion from that time until now has been played by the limestone formations, under the solvent action of drainage-waters acting everywhere through them, down to and for some depth below sea-level. Everybody knows the result in the great Appalachian valleys. Everybody knows how the Ohio valley region is undermined. I venture little in asserting that the new oil and gas region of north-western Ohio is thus undermined. This makes it essentially characteristically different from the Pennsylvania, West Virginia, and eastern Ohio oil and gas region. If the numerous wells bored at Findlay and in the twenty-seven counties of north-western Ohio have none of them struck through the roof of a mammoth cave, that negative argument is of no force when one calculates the chances of a well being drilled directly over such a cave. These caverns are the great underground drainage-channels. They correspond to the large streams on the earth's surface. What would be the chances for and against a man in a balloon at night dropping a bag of ballast into a river? A river, however, is a mile, half a mile, a furlong wide; a cavern cannot be more than fifty or one hundred feet wide. On the other hand, the caves are probably somewhat more numerous than the large surface-streams, but not much. Certainly no one will venture to deny the undermined condition of Ohio, until as many thousand wells have been bored into the Trenton formation as have been bored into the Pennsylvania Devonians.

But the underground drainage is only collected into and passed through the mammoth caves to some exit. Its collection takes place through the infinite multitude of vertical fissures which cut up the whole

limestone formation into blocks; and these fissures are all widened by chemical solution. The whole Trenton underground of Ohio must be like the Roman arsenal works at Baix,—a sort of crypt, in which water stands now at a level in the caves and fissures, because it can find no rapid issue at sea-level. In central Kentucky the cave-waters flow, because they can issue in the bed of the Ohio River; but in north-western Ohio the top of the Trenton Dr. Orton shows to lie from three hundred to nine hundred feet below sea-level (i.e., in round numbers, from eight hundred and fifty feet to fourteen hundred and fifty feet below the surface of Lake Erie), and therefore no flow is possible. The water must be standing water: the oil will therefore rise to its surface, and the gas press upon the surface of the oil, and over the whole extent of communicating fissures and caves equally.

But how could caves be formed at such a depth beneath sea-level? Standing water may corrode, but cannot erode. No one dreams that our Silurian caves in Pennsylvania follow the limestone strata many thousands of feet beneath sea-level under the great synclinals. No; but there are some wonderful facts for all that. There is a stream in Brush valley which sinks and flows under Nittany Mountain to rise in Nittany valley, where it drives a large mill. Sawdust and other things—a miller's hat among them—have made the underground voyage. The top of the limestone lies beneath the mountain two thousand feet lower than its outcrop and one thousand feet beneath ocean-level. It is an inverted siphon, with one mouth several hundred feet higher than the other; the confining top wall of the siphon being impervious Utica clay slate. At the Roman baths near Zurich (Baden in Aargau) river-water descends from a vertical outcrop to a depth of three thousand feet, and ascends, mineralized and heated, to the bath houses. The hot springs of Virginia are similar deep inverted limestone siphons.

In fact, there is no such thing as standing water anywhere. All water flows. Mere evaporation at one end of a canal will cause a current to set from the other end. Hydraulic pressure from the surface of middle Ohio will suffice to produce a universal lateral and upward water movement in northern Ohio, the Trenton sinking in that direction. If the currents thus induced be infinitely slow and gentle, nevertheless there has been an infinite amount of geological time (since the coal age) for them to effect their underground erosion in.

To all this must be added the great depth of the real rock-basin of Lake Erie. It is now only two hundred or three hundred feet deep; but who knows the thickness of its lining deposits? It has been receiving the inflow of Michigan, Indiana, Ohio, Pennsylvania, and the Canada peninsula for an unknown length of time, and, in addition to previous deposits, the glacial drift and modern river-muds. My belief is that its mother rock-bottom is excavated to a depth greater than the deepest wells of the new oil and gas region; and, if so, then the origin of the system of mammoth caves and fissures which hold the salt water, oil, and gas of north-western Ohio is relieved of difficulties. The water which is now nearly stagnant, flowed then freely to its natural outlets; the underground erosion which is now infinitesimally feeble, proceeded then energetically, at a rapid rate and on a grand scale.

What I wish to draw attention to is this: granting

a cavernous condition of the Trenton in Ohio, then Dr. Orton's terrace structure of the top of the Trenton becomes of value as indicating slopes in the general plane of the cavernous part of the formation. By this I mean to indicate the probability that the whole formation is not cavernous to an equal extent throughout (from top to bottom), but that certain members of the mass are more soluble than the rest. In Pennsylvania the Trenton itself is not cavernous on a grand scale: our sinking springs are along the outcrop of the passage-beds at the bottom of the Trenton and Bird's-eye and top of the calciferous. The whole formation in front of the Alleghany Mountains is between six and seven thousand feet thick. The uppermost thousand feet is very compact and non-magnesian; the underlying mass is composed of alternate layers of limestone and dolomite, with some low-lying calcareous sandstone groups. Dr. Orton reports the formation in Ohio 'magnesian, of a fair character throughout most of its extent,' but 'somewhat siliceous in some of the drillings.' It will be an important item of investigation, how far the cavernous horizon in Ohio corresponds to that in Pennsylvania, where the formation is ten times as thick as in Ohio. Dr. Orton inadvertently remarks (p. 18) that 'there is no warrant for assuming its universality as a limestone' under the country between middle Ohio and middle Pennsylvania. But I am sure that he will revise the remark when he reflects that a formation which is 'universally limestone' from Tennessee to the Manitoulin Islands in a north and south direction, and is universally limestone along the whole Appalachian belt from Alabama to New York, cannot possibly be any thing else than limestone under the intermediate region of the bituminous coal-measures. If there is any reasoning from the exposed to the concealed in geology, all geologists must feel sure—quite sure—that the lower Silurian formation No. II. must underlie Wheeling and Pittsburgh as a limestone formation, non-magnesian at top, magnesian at middle and bottom, at least two thousand if not three thousand feet thick, and at a depth of, say, ten thousand feet beneath the present surface.

But I have been led on to a much greater length than I expected, by the importance of the subject, to the new gas and oil industry of Ohio. I cannot trespass longer on your space with the obvious applications of what I have adduced above to the vexed questions of local capriciousness, etc., in the new oil and gas field.

J. P. LESLEY.

Philadelphia, Sept. 1.

The law of volumes in chemistry.

The questions regarding the so-called molecular weights and volumes of liquids and solids, which are now attracting the attention of chemists, can, I think, be better understood if we keep in mind the principles enunciated by the writer in 1853, that "the doctrine of chemical equivalents is that of the equivalency of volumes," and that "the simple relations of volumes which Gay-Lussac pointed out in the chemical changes of gases apply to all liquid and solid species;" so that "the application of the atomic hypothesis to explain the law of definite proportions becomes wholly unnecessary." In further illustration of this view, it was said in 1867 that "the gas or vapor of a volatile body constitutes a species distinct from the same body in a liquid or solid state; and

the liquid and solid species themselves often [probably always] constitute two distinct species of different equivalent weights." From this it follows that freezing, melting, and vaporization are chemical changes. The union of many volumes of a vapor or gas in a single volume of a liquid or a solid is a process of chemical combination, while vaporization is chemical decomposition. Such decomposition is either with or without specific difference, and examples of these two modes are seen respectively in heterogeneous dissociation and in integral volatilization, which latter is the breaking-up or dissociation of a polymeric species into simpler forms having the same centesimal composition. Both of these processes are subordinated to the same laws of pressure and temperature, and involve similar thermic changes in the relations of the bodies concerned. In this enlarged conception of the chemical process we find a solution of the problems above named, and an explanation of the distinction which has been made between 'the chemical molecule' and 'the molecule of the physicist.' That the latter has a much less simple constitution than the former, as calculated from the results of chemical analysis and from vapor-density, has been long maintained alike on dynamical and chemical grounds. It is discussed by the writer in 1853 in the essay already quoted, entitled 'The theory of chemical changes and equivalent volumes,'¹ and again in the late paper of Spencer Pickering in the *Chemical news* for November, 1885.

If, then, as maintained by the writer, the law of volumes is universal, and if the production of liquids and solids by the condensation of vapors is a process of chemical union giving rise to polymerids, the equivalent weights of which are as much more elevated as their densities are greater than those of the vapors which combine to form them, the hypothesis of atoms and molecules, as applied to explain the law of definite proportions and the chemical process, is not only unnecessary, but misleading. According to this hypothesis, which supposes molecules to be built up of atoms, and masses of molecules, the different ratios in unlike species between the combining weight of the chemical unit or molecule (as deduced from analysis and from vapor-density; $H = 1.0$) and the specific gravity of the mass are supposed to represent the relative dimensions of the molecule. Hence the values got by dividing these combining weights by the specific gravity have been called 'molecular volumes.' The number of such molecules required to build up a physical molecule of constant volume would, according to this hypothesis, be inversely as their size. If, however, as all the phenomena of chemistry show, the formation of higher and more complex species is by condensation, or, in other words, by identification of volume, and not by juxtaposition, it follows that the so-called molecular volumes are really the numbers representing the relative amount of contraction of the respective substances in passing from the gaseous to the liquid or solid state, and are the reciprocals of the coefficient of condensation of the assumed chemical units. If steam at 100° C. and 760 millimetres pressure, with a formula as deduced from its density of H_2O , and a combining weight of 18, is converted into water at the same temperature, 1,628 volumes of it are condensed into a single volume, having a specific gravity of 0.9588, which at 4° C. becomes 1.0000. Water is

¹ See the author's 'Chemical and geological essays,' pp. 426-437, and, further, *ibid.*, pp. 453-458.

thus 1,628 (H_2O); and the weight of its volume at the temperature of formation, as compared with an equal volume of hydrogen gas or of steam, in other words, its equivalent weight, is $1,628 \times 18 = 30,304$, which thus corresponds to a specific gravity of 1.0000; ice, at its temperature of formation, with a specific gravity of 0.9167, being 1,487 (H_2O) with an equivalent weight of 26,766. The hydrocarbon, $\text{C}_4\text{H}_{10} = 58$, condenses to a liquid having, according to Pelouze and Cahours, a specific gravity of 0.600, which corresponds to an equivalent weight, as compared with that of water, of 17,582, or approximately 303 (C_4H_{10}), with a calculated specific gravity of 0.5997. The reciprocal of the coefficient of condensation (or so-called molecular volume) of steam is 18, while that of the gaseous hydrocarbon is $600 : 1000 :: 58 : x = 96.66$.

The chemical unit for bodies, which, like these, volatilize integrally, is fixed by the density of their vapors; while for fixed species, like anhydrous oxides and silicates, or for those which by heat undergo heterogeneous dissociation, as for example calcic and hydrous silicates, the unit may be the simplest formula deduced from analysis, or, for greater convenience in calculation in the case of oxides and silicates, may have a value corresponding to $\text{H} = 1$, or $\text{O} = 8$. The unit for silica thus becomes $\text{SiO}_2 \div 4 = 15$; that for alumina, $\text{Al}_2\text{O}_3 \div 6 = 17$; and that for the magnesian silicate, $\text{SiMg}_2\text{O}_4 \div 8 = 17.5$. Such unit-weights as these have been employed by the writer in his late essay on 'A natural system in mineralogy,' in the tables of which they are represented by P; while the values got by dividing these numbers by the specific gravity of the species have been designated unit-volumes, and represented by V. The writer of that essay, in deference to the general usage of chemists, therein adopted the received terminology of 'molecular weights' and 'molecular volumes,' and, failing at the time to grasp the full significance of his own earlier teachings as to the universality of the law of volumes, spoke of the so-called molecular weight as an unknown quantity, although in accordance with that principle this molecular weight, or, properly speaking, this equivalent weight, is simply deduced for any body the specific gravity of which is known.

T. STERRY HUNT.

Centre Harbor, N.H., Sept. 3.

The old gorge at Niagara.

The existence of a drift filled channel running from the west side of the whirlpool on the Niagara River, to the wide, open valley of St. David's on the north face of the Silurian escarpment, has been known to geologists ever since the publication of Sir C. Lyell's 'Principles of geology.' It was considered by him as an ancient channel of the river, and it has been so regarded by many geologists ever since. Arguments numerous and of no slight weight can be quoted in favor of this opinion. But of late years it has been somewhat modified, and a disposition has been manifested to regard this drift-filled valley of St. David's as consisting of two smaller valleys, one of which was excavated by a stream flowing into the place of the present whirlpool, and the other into the valley of St. David's. On the latter theory there may be a solid barrier of rock not far underground between the two valleys. In the latter no such bar can exist.

Into the discussion of this subject I will not now enter. It would require more time and space than

can be afforded. I desire merely to mention a single fact. In the course of the arguments on this point it has been apparently taken for granted, if not asserted, that no rock can be seen in place along this gorge, but that it is filled deeply with drift almost from end to end. During the recent meeting of the American association I took an opportunity of going up the valley from the whirlpool, and was much surprised to find a ledge of limestone exposed at its bottom about a hundred feet above the river. On both sides it disappeared beneath the talus, but probability indicates its continuance from side to side, especially as a considerable surface is exposed. This point can only be decided by quarrying.

The importance of a bed of limestone so situated, on the discussion of this question, is obvious. It does not seriously affect the latter of the two hypotheses mentioned above, which is, however, beset by other grave difficulties. But in regard to the former it proves, that, if the Niagara River ever passed that way, its bed was far above the present level. No concealed side-channel can be admitted in this case. The space is too small. A line of drill-holes carried along the course of the valley can alone supply the evidence needed for a decision between the two rival theories.

It is scarcely necessary to point out the bearing of this fact on those calculations of the age of the great gorge which assumes that any part of it above the lower rapids was merely cleaned out and not excavated from solid rock since the end of the ice age.

E. W. CLAYPOLE.

Science for a livelihood.

Some time ago I read in your journal a stirring editorial, calling for young men to devote their energies and life to the cause of science, and deploring the lack of persons who were willing to encounter hard work and poor pay because of love for investigation and study.

Early this summer, after graduating from a first-class scientific school, I made application to four agricultural stations in this and other states for some position, pay no consideration whatever. Having been brought up on a farm, and having a first-rate scientific education, a love of the natural sciences (in which I have done a little practical work), and an excellent physique, I thought myself fitted for investigation in scientific fields, particularly as I love it above all else.

In every case I received answer, 'Places all full.' I have begun to doubt if investigators and workers are needed in the natural or experimental sciences, and think that a poor young man who cannot afford to give money to the work has no call in this field. Am I right?

C. B.

Brooklyn, N.Y., Sept. 4.

Revivification.

In answer to your Paris correspondent, I would say that quite recently, a native of India, after his conversion to Christianity, gave an exhibition and full explanation of the trance, as I am informed by a missionary just returned from that country. Full particulars can be obtained by addressing Rev. S. Knowles, Gonda, Province of Oude, India.

E. T. NELSON.

Ohio Wesleyan university, Sept. 6.

SCIENCE.—SUPPLEMENT.

FRIDAY, SEPTEMBER 10, 1886.

AN EASY METHOD OF MEASURING THE TIME OF MENTAL PROCESSES.

It is justly considered one of the triumphs of physiological psychology to have made the elementary processes involved in perceiving and thinking more real and better known, by comparing the times necessary for their performance. It has made the connection between mental action and the function of the brain closer, by showing that all processes take time, and that this time is varied by abnormal conditions of the brain. These psychometrical observations, though of but recent date, form one of the favorite fields of present psychological research.

The usual method of measuring one's reaction time is somewhat as follows: The subject is seated with his hand in contact with an electric key: his attention is to be directed, we will say, to a flash of light electrically produced before him. The operator controls the appearance of the spark by simply breaking an electric connection: at the same instant he sets in motion (by the same current) a Hipp chronoscope,¹ which in turn is stopped immediately on the closure of the key by the subject. The interval during which the clock was recording will then be the time necessary for the subject to perceive the light. But in this time several elements are involved. These can be separately investigated by other means. We have, 1°, a series of afferent processes, such as the time necessary for the sense-organ (in this case the retina²) to be affected, the time necessary for the impulse to travel along the sensory nerves to the brain; 2°, the reception of the sensation in the brain (plus, perhaps, the generation of the will); and, 3°, a series of afferent phenomena, including the transmission of the impulse from the brain to the spinal cord, down the cord to the anterior nerve-roots, thence along the afferent nerves to the muscles, the latent time of the muscles, and, finally, the contraction of the muscles closing the key. The phenomenon in which the psychologist is interested

¹ This is an instrument which, by a clock-work arrangement, records to the thousandth of a second. It is set in motion electrically by the release of a magnet, and stopped by the closure of the same. A tuning fork recording on a revolving drum, or similar arrangement, is often used in its place.

² If the stimulus excited the touch, we should also have the time for transmission along the nerve to the spinal cord, and the slow travelling up the cord.

is included under 2°. But to determine that, he must eliminate 1° and 3°. And here we see how essentially physiological a real psychology is: it has need of facts which none but a physiologist would undertake to discover. We want to know the rate at which the nervous impulse travels. This Helmholtz measured in 1850, only a few years after Johannes Müller despaired of our ever ascertaining it, and found to be about 33 metres (108 feet) per second for both motor and sensory nerves. The travelling along the cord is much slower,—about 10 metres (33 feet) per second. The very minute times involved in the delay in the sense-organ, ganglion of the spinal nerves, and muscle, have also been accurately determined. The whole operation, i.e., the complete reaction time, takes about $\frac{1}{4}$ of a second, of which the process included under 2° consumes a share subject to great variation according to the conditions of the experiment, but always small.

Let the operation be somewhat more complex. Say that the light shall not always be of the same kind, but that at times it shall be red, and at times blue. The subject is not to react until he has perceived the blueness or redness of the light. If we subtract the simple reaction time from the total time intervening between the appearance of the colored light and the closing of the key after the subject has seen whether it is a red or a blue light, we shall have the time required to distinguish red from blue. This we will call the 'distinction' time. We can evidently make the distinction more difficult by having three, four, or more colors. The average distinction time between two sensations, though largely variable, is about from $\frac{1}{17}$ to $\frac{1}{25}$ of a second, or less.

In the above experiment it has been assumed that the nature of the reaction has remained unaltered; that is, in each case the subject closed the one key before him. This, too, is capable of complication. We can agree that the subject is to react by a key on his right hand if a red light appears, and by one on his left if a blue light appears. If we subtract the time necessary for all the processes up to the color distinction from the time required to close the appropriate key, we shall obtain the time necessary for making a choice between two reactions. While before we were testing the readiness of the subject's sensibility and of his judgment, we are now testing the alertness of his will. That time necessary for this new process we will call the 'choice' time. According to

Wundt, it is a little longer than the distinction time, and, like it, is very much affected by different conditions of mind, and varies largely in different individuals. It, too, can be complicated by making the choice between three, four, or several modes of reaction.

Only one more type of reaction time will be here mentioned. It is called an 'association time,' and is measured as follows: A word is called, and simultaneous with the call the clock-work is set in motion. As soon as possible after the word is heard, the subject answers by uttering the first word associated with the call-word that suggests itself to him. By subtracting from this time the time necessary for the hearing of the first word and the utterance of the second, we have the time involved in the process of association, or the 'association time.' This is a very much more complicated process, and naturally occupies a longer time, — about $\frac{3}{4}$ of a second. It differs largely in different states of mind and in individuals. It can be complicated by restricting the kind of words allowable as associations. For example, only words related to the call-word as part to whole may be allowed. We thus test what may be termed the 'suggestiveness,' or co-ordination, of one's mental furniture.

All these reaction times have been measured in laboratories under somewhat artificial conditions, and with the aid of more or less elaborate apparatus. It has long been desirable to avoid this artificiality, and thus make the inferences from such experiments to similar processes in our daily thought more certain and immediate, and to simplify the apparatus so that the demonstration of these mental times may be easy and inexpensive. It is to describe an attempt at solving these difficulties with reference to a few types of reaction times, that I devote this article.

My method is a very simple one. We require delicate apparatus, because we have to measure very small fractions of a second; and this, in turn, is necessary, because we measure but a single reaction time at once. By measuring a long series of successive reactions we can dispense with delicate time apparatus; for the error involved by such neglect will be divided among the whole series, and will thus not appreciably affect the value of the average reaction time. For our purposes a small clock or a watch beating quarter-seconds, as a rule, is sufficiently accurate. One can readily count four to the second, and the process can be made still easier by tallying off the 'tens' by pencil-marks or on one's fingers. It is advisable, in counting, to emphasize alternate numbers; thus, one, *two*, three, *four*, five, *six*, etc. We shall find incidentally that the conditions

suitable for such experimentation are unconstrained and natural. The method is applicable to all the kinds of reaction times above described.

1. *Simple reaction times.* — Here I have but a single experiment to offer. On one occasion I imposed sufficiently on the good nature of an evening company of about eighteen persons to ask them to arrange themselves in a circle, each one standing with the forefinger of one hand resting upon the shoulder of the person before him. At a given signal, one of the party gently pressed with his finger upon his neighbor's shoulder, who in turn communicated the impression as soon as he felt it to the shoulder of the one before him; and so on around the circle. The impression made four or five complete revolutions, and the time was taken to the nearest quarter of a second. By dividing the time by the product of the number of revolutions of the impression into the number of persons, one obtains the average simple reaction time for a touch impression. A little drill would be necessary before the time would be constant, inasmuch as a miscellaneous set of persons do not readily act together without rehearsals. My time was about $\frac{1}{3}$ of a second, but it would evidently have been shorter could I have repeated the experiment. It is recommended as a useful evening amusement. There is one point more: if the reaction time of any particular individual is desired, one has only to subtract from the average time of one revolution of a circle in which he forms a member, the time of a revolution of the impression in which he is absent.

2. *Distinction time.* — The apparatus consists of a clock ticking quarter-seconds (a stop-watch is much more convenient), and several packs of ordinary playing-cards. To begin with a very simple case: Take a single pack of cards; throw out all the face cards, and you have forty cards left, of which twenty are red, and twenty black. Shuffle these well together. Let the assistant be ready with the clock close to his ear to give you a signal when to begin, and to count the ticks. The 'one' by which he begins his counting is a good signal.¹ The moment you hear the word 'one,' you throw the first of the forty cards upon the table, and continue to do so with the rest, distributing them into two heaps. As you throw the last card, you call 'Done!' whereupon the assistant closes his counting. The cards must be divided without any plan between the two heaps — about as a chance arrangement would divide them. The time consumed in this operation divided by the number of cards will be spoken of as the 'throwing time.'

What naturally suggests itself as the next opera-

¹ It is advisable to prepare the subject for the signal by previously calling, 'Ready!'

tion is to repeat the process by which the throwing time was obtained, with the difference that the card is not to be deposited before the thrower has appreciated the color, whether red or black, of each card to be thrown. The time necessary for this process, minus the throwing time, would be the time which it took the person to distinguish red from black. But this method is really not valid at all, and for the following reason. While throwing one card, one can in the indirect field of attention, so to say, be preparing to decide or already deciding what the color of the following card is; so that the two operations of throwing and distinguishing partly overlap. A distinction time gotten by such a proceeding would be entirely too short. Several ways of avoiding this difficulty were suggested, of which I used the following one. The cards were held with the backs towards the thrower. The operation consisted, first in simply turning the card with its face upward, and depositing it on a heap; and, second, in not depositing it before its color has been seen. In this way the person cannot see the following card, because it has its back towards him; and all the cards may be placed on a single heap. The average difference between the time required for the first operation and that for the second, divided by the number of cards, will give the distinction time for distinguishing red from black.¹

I have described the simplest type of a distinction time. The process can be indefinitely complicated by having three, four, or more colors to distinguish, using the backs of variously colored cards, or by distinguishing the four suits of one pack. By having several packs of cards, one can vary the experiments in very many ways. One can distinguish as many of the spot-cards as one pleases, from two to ten; can, in addition to this distinction, distinguish between the suits; and so on. Before giving the results I have obtained in this way, I will anticipate the question whether the number of cards used will not affect the result. It probably will; for the mind, being once set on the habit of making these distinctions, can keep up the process with less energy, and thus with greater rapidity. This question I hope to solve by a special

¹ In another method the forty cards are spread out upon a table, say, in five rows of eight each. The subject first runs his eye along each row, going forward on one row and backward on the next, dwelling on each card only long enough to bring it into distinct vision. The operation is very rapid (being faster than counting), but is rather uncertain. Next, he 'reads' the color of each card in the same manner. The difference between the times necessary for these operations evidently, again, gives the distinction time. Here, too, reading ahead in indirect vision is possible, but not to any great extent. The method is of value only as a means of checking the results of the first method, but is inferior to it. Doubtless some of my readers will invent a method better than this or the one described in the text.

set of experiments. From what I have done I am able to say that the variation will be extremely slight. It is recommended to use forty or sixty cards, as it is easy to hold that number in one's hand, and these numbers are divisible by 2, 3, 4, 5, and the latter by 6. Moreover, $\frac{1}{40}$ or $\frac{1}{60}$ of the error involved in neglecting fractions of a second less than a quarter is a small error indeed.

The persons whose reaction times were taken were, I., a girl of ten years; II., a young lady and, III., myself. In all the experiments in which II. and III. took part sixty cards, and in all in which I. was the subject forty cards, were used. In the following table the time is always given in seconds.

| Subject. | 5's f'm 9's 2's " 4's etc. | 2's, 4's, 6's. | 2's, 4's, 6's, 8's. | 2's, 4's, 6's, 8's, 10's. | 1's, 2's, 4's, 6's, 8's, 10's. | Green from blue. |
|----------|----------------------------------|-------------------|------------------------|---------------------------------|--------------------------------------|------------------------|
| | | | | | | |
| I. | .058 | .097 | .159 | .250 | — | .036 |
| II. | .045 | .073 | .078 | .089 | .110 | .037 |
| III. | .043 | .054 | .061 | .068 | .074 | .031 |

The column headed 5's from 9's, 2's from 4's, etc., indicates that the pack of cards was divided equally between two-spots and four-spots, or five-spots and nine-spots, or some similar combination of two kinds of cards; and that the subject had to distinguish by the method above described the denomination of each card. It thus appears that it took I. .058 of a second to make this distinction, and II. and III. .045 and .043 of a second respectively. In other words, it takes $\frac{1}{23}$ of a second to tell whether a card is a five-spot or a nine-spot, or to make any similar distinction. The only experiment performed by the usual laboratory methods, which I could find, comparable with this, was one by Professor Wundt, undertaken in his psychological laboratory at Leipzig, in which the distinction was made between a black cross on a white background, and a white cross on a black background. He gives .0485 of a second as the distinction time, which agrees well with .044, the average of the times of the two adults in the above table. The distinction between the green and blue backs of cards, as is shown by the last column of the table, is more rapidly made. Perhaps part of the difference is due to the fact that the card did not need to be turned so completely around to see the color as to see the denomination.

In the other columns of the table is shown the result of a series of experiments in which the cards were divided among three, four, five, or six kinds, as indicated in the heading. It is seen, that though the thing to be done remains the same,

namely, to read the denomination of each card, yet it takes longer to do so the greater the number of denominations to which it may belong.¹ One must take a longer look at a card to tell that it is a four-spot, for instance, where it may be a one, two, four, six, eight, or ten, than when it may be a two, four, or six. This difference was most marked with me in passing from two to three kinds. The increasing number of possibilities is more puzzling to the little girl than to the others; for it takes her as much as $\frac{1}{4}$ of a second to tell the cards when five denominations are used, whereas it takes the others only about $\frac{1}{18}$ of a second.

A few words of caution must be added for those who intend to repeat the experiments. Do not expect very constant results at first; the familiarity which one acquires after the second or third trial very much reduces the time; after this there is a more gradual reduction, due to practice. The numbers in the table are regular only because founded on many sets of experiments, and the first few records of each kind of reaction are omitted in a few cases.

3. *Choice time.*—This time is obtained by an indirect process. We have already become acquainted with the 'throwing time.' This time has no particular psychological interest, as it simply tells how long it takes one to throw out cards. This time will differ very largely in different persons, and is much reduced by practice. It took I. $\frac{2}{5}$ of a second, II. $\frac{3}{10}$ of a second, and III. $\frac{1}{10}$ of a second, to throw a card upon one of two heaps. It takes longer to distribute the cards, the more numerous the heaps among which they are to be divided; but the increase in time is slight. It took I. less than $\frac{1}{2}$ of a second to place a card in one of five heaps, and II. and III. $\frac{2}{5}$ and $\frac{1}{5}$ of a second respectively when six heaps were used. Of course, the time refers to the simple operation of placing the cards, without reference to their denomination, in one of a certain number of heaps. Each of these counts has a different mode of reaction.

Having gotten the throwing time, the next step is to distribute the cards among the heaps in such a way that each heap will contain but one kind of cards. If we are throwing five-spots and nine-spots, then all the five-spots must be put on one heap, and all the nine-spots on the other. If we are using two, four, six, and eight spots, then there will be four heaps, each containing all the cards of one denomination. In addition to the time con-

sumed by the manual operation of taking the card and placing it on the pack, part of the time is consumed in recognizing the denomination of the card, and the rest in placing it on its appropriate pack. In other words, if from the time occupied by this operation we subtract the throwing time, we have left the distinction time together with the choice time. But we know the value of the distinction time by our previous experiments. Simple subtraction yields the choice time. I will again put the results in the form of a table.

| Subject. | 5's, 1'm 9's | 2's, 4's, | 2's, 4's, | 2's, 4's, | 1's, 2's, | Green from blue. |
|----------|-------------------|-----------|-----------|--------------------|-------------------------|------------------------|
| | 2's " 4's etc. | 6's. | 6's, 8's. | 6's, 8's, 10's. | 4's, 6's, 8's, 10's. | |
| I. | .062 | .100 | .193 | .353 | — | .058 |
| II. | .045 | .117 | .144 | .162 | .169 | .050 |
| III. | .029 | .089 | .095 | .098 | .100 | .032 |

If we compare this table with the former one, we see at once that, as before, the time increases with the complexity of the operation; but the increase is more rapid in this table than in the former one. This is just what we should expect; for in the former case it was the same process to be done under different conditions, while here the nature of the reaction is changed with each additional kind of card. When we deal with but two kinds of cards, the choice time and the distinction time are about equal. This agrees well with Professor Wundt's results.¹ The process readily becomes at least partly automatic. But as we pass to a choice between three kinds of reactions, it would seem that a distinct exertion of the will is necessary in each case. The time undergoes a marked increase. From that point on, the increase in time with the complexity of the operation is more gradual. But, as before, the little girl finds great difficulty in distributing the cards appropriately when many kinds are used. It takes her over $\frac{1}{5}$ of a second to determine upon which of five heaps to put a card after she knows its denomination, while it only takes the others $\frac{1}{5}$ and $\frac{1}{10}$ of a second to perform the same operation with six heaps.

A comparison of the first and last columns of the table shows the regularity of the phenomena we are studying. The choice time ought not, of course, to be affected by the nature of the distinction upon which it is founded; and the choice time for five-spots and nine-spots and that for green and blue ought to be and are (approximately)

¹ The only comparable experiment (and the similarity is not very close) I can find is one recently published by Dr. Cattell, in which he finds that it takes only about 1-160 of a second longer to distinguish one out of ten than one out of two colors.

¹ It is again difficult to find comparable results. But the distinction plus choice time can be compared with similar results of Dr. Cattell. His figure is .078; mine is 0.81.

alike. We thus have a means of varying one without the other. The independence of the two processes (distinction and choice) is further shown by the fact that II. is the quickest distinguisher, while III. is the most ready chooser. III. is slowest in both operations, but differs less in the readiness of her sensibility and judgment than in the alertness of her will. Perhaps an educational truth with regard to the development of the mental powers is hinted at here.

4. *Association time.*—Here our apparatus reduces itself to a clock and some slips of paper; but the number of persons involved in the experiments must be increased from two to three. Let each of the three write on the slips of paper ten or twenty words, say, of one syllable each, and the names of concrete things. Avoid any natural connection between the words by not writing them in the order in which they were thought of. Now let I. and II. be the subjects of the experiment, while III. records the time. 1°. Let I. begin by calling, as soon as he hears the signal, the first word on his list: hereupon II. answers by the first word which he can associate with the call-word, and immediately upon this calls his first word to I., who in turn performs the association and calls his second word; and so on to the end. If there are ten words on the list of each, then each person has called ten words, has answered ten words, and has performed ten associations. 2°. Now let I. and II. each have twenty words before him, and let each call a word as soon as he hears the answer of the other.¹ This operation will differ from the former only by the fact that the association has been omitted. The difference in time between 2° and 1° divided by 10, will give the sum of the association times of I. and II.

Now let I. and III. be the subjects, and II. take the time, and the sum of the association times of I. and III. will be obtained. Then get the sum of the times for II. and III., and the solution of a very simple algebraic equation will give the value of the association time of each.

I have also used another, perhaps somewhat simpler method. It differs only in that in each operation one person acts as caller, and the other as associater, throughout. In this way the values of six equations are gotten: i. e., I. (caller) + II. (associater) = ?; II. (caller) + I. (associater) = ?; and so with each pair. We then eliminate the value of 'I. (caller),' 'II. (caller),' etc., by getting the value of the three equations, — 'I (caller) + II. (caller),' 'I. (caller) + III. (caller),' etc., just as before. The results of the two methods agree very well, and one may be used as a check upon

the results of the other. The effect of practice in reducing the time is at first very considerable.

It remains to be noted, that after I have ascertained my own association time and my own calling time, and know it to be fairly constant, the work of finding the reaction time of a fourth person is much reduced. We have simply to get the sum of our association times and of our calling times, and subtract from these my own association and my own calling time.

I will give the results of the first method, because here alone is the effect of practice (in two of the subjects) eliminated. The subjects are the II. and III. of our former experiments, and the times are .803 and .872 of a second respectively, which agrees very well with .764 of a second, which is the time found by Professor Wundt by the more elaborate methods. The great difference between this time and that necessary for a distinction or a choice, shows how much more elaborate the former process is.

The methods above described leave much to be desired; but the principle upon which they depend (namely, of substituting a series of reactions for a single one, and of arranging the apparatus so that the subject himself produces the sensations upon which the distinction and choice is made) seems to be the one by which the desired simplification can be best accomplished. If the above account shall be the means of setting others to work at the same problem, and of popularizing to any extent the study of experimental psychology, its object will be more than fulfilled. JOSEPH JASTROW.

THE HYGIENE OF THE VOCAL ORGANS.

THE experience which Dr. Mackenzie has had for the past twenty-five years, as a specialist in the treatment of diseases of the throat, renders him thoroughly competent to advise on the important subject of which he treats in the volume before us. Additional interest attaches to his utterances for the reason that during this active career, the most famous singers have come under his professional care and observation, including Nilsson, Albani, Vallina, Patti, and a host of others.

Dr. Mackenzie well says that hygiene has a positive as well as a negative side. The preservation of health means not only that actual mischief is avoided, but that the body is kept in the best working order. The hygiene of the voice, therefore, must include a consideration of the best methods of developing its powers to the highest

¹ The words should be pronounced distinctly, and no more rapidly than in the first operation.

The hygiene of the vocal organs; a practical handbook for singers and speakers. By MORELL MACKENZIE, M.D. London, Macmillan, 1886. 12^c.

pitch as well as protecting it from injury or decay.

After describing the anatomy of the vocal organs, the author passes to a consideration of the uses of the laryngoscope. Although this instrument is of inestimable value in the recognition and treatment of disease, it has, nevertheless, added very little to the knowledge of the physiology of the larynx. This is accounted for by the greater amount of skill required for the examination of the larynx in the act of singing than for ordinary medical purposes, and also by the fact that but few throats are sufficiently tolerant to permit of such a prolonged examination as is necessary to obtain results of much value.

The development of the voice receives considerable attention in the author's methods. Many children can be taught to sing little airs when they are between three and four years old. From the age of six until that of fourteen or sixteen the voice undergoes but little change except in the way of gaining power. At this time a marked change occurs, more noticeable in boys than girls, that is, 'the changing of the voice.' This is due to an increase in the size of the larynx in all its dimensions, enlargement and consolidation of the cartilages, and an increase in length and thickness of the vocal cords.

In speaking of the training of the singing voice, Dr. Mackenzie recommends vocal gymnastics and a development of the breathing capacity, by walking, hill-climbing, running, fencing, and swimming, and in a chapter devoted to the care of the formed voice directs the avoidance of strain and complete inaction of the vocal organs when out of order. The influence of the general health upon the voice is very marked. Whatever is good for the singer's general health is *pro tanto* beneficial to his voice. Alcohol and tobacco should not be used. The hoarse tones of the confirmed votary of Bacchus are due to chronic inflammation of the lining membrane of the larynx: the originally smooth surface being roughened and thickened by the irritation of alcohol, the vocal cords have less freedom of movement, and their vibrations are blurred, or rather muffled, by the unevenness of their contiguous edges.

In discussing the speaking voice, its compass, mechanism, and defects are fully considered. The various diseases of the larynx, paralysis, and abnormal growths are not overlooked, and a special chapter treats of the training of the voice for speaking in public. The importance of early training is dwelt upon, and the improvement which is possible to a poor voice by proper methods of culture.

In concluding the volume, Dr. Mackenzie de-

sires it to be understood that he speaks as a physician, rather than as a singing-master or an elocutionist, and that his aim is to furnish the vocalist and public speaker with a guide to the diseases of the voice, and the best means of avoiding them. He has accomplished his object in a manner which is no surprise to those who know his skill and acquirements.

RECENT EARTHQUAKE LITERATURE.

Report on the East Anglian earthquake of April 22, 1884.
By R. MELDOLA and WILLIAM WHITE. London, 1885.

THE Essex field-club of England has devoted vol. i. of its 'Special memoirs' to the Essex earthquake of April 22, 1884, which has already been the subject of sundry articles in scientific periodicals and society transactions. This publication is much the most extended discussion of the phenomena which has appeared, and its authors have here given us an excellent example of the thorough presentation and discussion of the facts observed. It forms a volume of two hundred and twenty-three pages, with four maps and numerous illustrations in the text. It begins by giving a list of nearly sixty previous British earthquakes which had caused structural damage, the records being drawn from various sources, and including some that are not mentioned in Mallet's 'British association catalogue.'

After describing the careful methods of collecting and sifting the data in regard to the present shock, some twenty pages are devoted to its general character. It is regarded as the most serious seismic disturbance that has affected Great Britain for four centuries, extended over fully five thousand square miles, and in intensity is estimated as about one-twentieth of the great Lisbon earthquake of 1755. Pages 44 to 155 are given up to a detailed description of the phenomena at various places, the accounts being in many cases in the original language of the reporter, and in many more giving the result of personal examination of the localities, immediately after the occurrence, by the authors themselves or by competent persons authorized by them. No one who has not himself engaged in similar work can understand the labor involved in the collection and arrangement of the materials here presented. They are accompanied by numerous wood-cuts illustrating the damage done to particular buildings, and the general impression produced by their perusal is that the shock was much more severe and destructive than the accounts published at the time had led us to suppose. Many instances are given of buildings so wrecked as to be uninhabitable, and in some towns the injured buildings are numbered by

hundreds. The whole number of buildings damaged was estimated between twelve hundred and thirteen hundred, including twenty churches and eleven chapels. The area of structural damage was confined to fifty or sixty square miles in north-east Essex, having its main axis in a direction north-east and south-west from Wivenhoe to Peldon.

In considering the connection of the shock with surface geology, the chief damage is found to have been upon the London clay; but some evidence was found that the shock was spread widely, especially toward the north and north-west, by the better conducting older rocks which lie underneath. The excessive damage at Wivenhoe, as well as its comparatively sudden decrease to the north-east of that place, is attributed to reflection of the earth-wave at that place by the valley of the Colne River.

Attempts to estimate the velocity of propagation, the exact location of the centrum, etc., are admitted by the authors to be of little value as to results, owing to the uncertainty of the data available in a country where earthquakes are rare, and therefore find no one prepared for careful observation, and where also seismographs are practically unknown; but they furnish further evidence of the care with which this report has been prepared.

Alphabetical catalogue of the earthquakes recorded as having occurred in Europe and adjacent countries. By J. P. O'REILLY. Dublin, 1886.

The second memoir named above forms a part of vol. xxviii. of the Transactions of the Royal Irish academy. It is arranged on the same plan as the similar catalogue of British earthquakes published in 1884, by the same author, and which forms an earlier part of the same volume of Transactions. The present list is based mainly upon those of Mallet, Perrey, and Fuchs, and aims to give, for each of the localities arranged in alphabetical order, the number of recorded earthquake shocks, with their dates and condensed indications of the extent of the area affected. It forms a volume of two hundred and twenty quarto pages.

As it is very difficult, even where all the facts are known, to make any numerical estimate of intensity (and, moreover, for the vast majority of recorded shocks no sufficient details are now available on which to base such estimate), the element of intensity has been omitted in preparing this list, and it represents only the number of recorded earthquakes, the unit adopted being the shock. When several or many or continuous shocks are recorded, these are interpreted as meaning two or three or four shocks,—estimates which are certainly usually within the truth. It is also recog-

nized that in the earlier centuries many earthquakes must have passed entirely unrecorded, and that the list is necessarily in so far an incomplete record of the true number of earthquakes that have occurred.

In Professor O'Reilly's former memoir concerning British earthquakes, an earthquake map of the islands was presented. The present list is not accompanied by a corresponding map of Europe, the preparation of which will necessarily involve considerable time and trouble, and which, we hope and expect, will in due time appear. In these days when the graphical representation of all physical phenomena has become so common, it is certainly an important advance in seismology to be able to apply the same methods to the study of earthquake frequency in various parts of the world; and we anticipate the day when a similar map of the United States may be available for American seismologists. Indeed, some progress has already been made by the present writer in the preparation of such a map.

Statistik der erdbeben von 1865-85. Von C. W. C. FUCHS. Vienna, 1886.

Dr. Fuchs's memoir is from the ninety-second volume (1885) of the Sitzungsberichte of the Vienna academy. In it he has collected the records of earthquakes from his various annual reports, and arranged them according to countries; so that the statistics for any particular locality for the whole twenty years are now easily available to the student. It forms another chapter in the general earthquake catalogue which Mallet brought down to 1843, and which Perrey continued for the later years. It would be a desirable thing if Perrey's lists for the years from 1843 to 1865, which are scattered in numerous separate memoirs, could be collected and collated in a similar way. In order that the lists for different countries may be comparable one with another, Dr. Fuchs has included in his present lists only those shocks which were sensible without instruments; that is, those which correspond to the numbers III to X in the Rossi-Forel scale. The lists proper occupy about four hundred octavo pages, and are preceded by a brief separate notice of the more important earthquakes. It would have increased somewhat the usefulness of Dr. Fuchs's book if he had added an index of the countries; but, as they are arranged geographically, the deficiency is not a serious one.

Transactions of the Seismological society of Japan. Vol. ix. part 1. Tokio, 1886.

Vol. ix. of the Transactions of the Japanese society opens with a paper occupying twenty-three pages, by Dr. C. G. Knott, on earthquake frequency. After a discussion of the probable length of any

periodicity which might be due to the gravitational action of the sun or moon, with the result that the periods most likely to be discoverable are semi-annual and annual, he gives a method of combining the monthly numbers so as to eliminate any shorter periods. The author then applies these methods to Mallet's list of European earthquakes, to New Zealand earthquakes (1869-79), to the East Indian Archipelago (1873-81), to Chili earthquakes (1873-81), and to the Grecian Archipelago (1859-73). The resulting numbers are plotted in two sets of curves; the one showing the annual period, the other the semi-annual period if there be one. The curves, excepting that for the East Indies, show clear indication of a semi-annual period, but the author finds reason to doubt whether it is to be attributed to the gravitational cause which suggested the search for it. In considering the possible effect of atmospheric changes, it is suggested that earthquakes, frequently at least, are not local phenomena, and their causes may be sought at a considerable distance from the place where they occur; as, for instance, changes of pressure over the continent of Asia or over the Pacific Ocean might cause variations in the strains along the littoral line between them, and so might be a determining cause of earthquakes in the Japanese area. Pursuing this idea, Dr. Knott finds a possible or probable cause of the winter earthquake maximum (which his annual curves show in both the northern and southern regions) in the accumulations of snow over continental areas and in the annual change of barometric gradients.

The remainder of part i. is occupied by Prof. R. Shida, with two papers, entitled 'Automatic current recorder,' and 'On earth-currents.' In the former he has described and figured an instrument for automatically recording the strength and direction of a varying electric current. In the latter he has collected a brief account of what has been done in the way of observing earth-currents, adding observations of his own which seem to indicate, that, while the magnetic declination and earth-currents vary in a similar way, the latter changes are not the cause of the former, inasmuch as an increase of western declination corresponds to an increase of current flowing from north to south along a telegraph-wire, not to a decrease, as should be the case if the connection were causal. The author also discusses the possible connection of earth-currents with earthquakes.

Transactions of the Seismological society of Japan. Vol. ix. part 2. Yokohama, 1886.

The second part of this vol. ix., separately bound, is entirely occupied by John Milne with an account of the volcanoes of Japan, mainly historical and descriptive. The author gives a list of over forty

works which have been consulted in its preparation, of which twenty-six are in Japanese, a considerable proportion being in manuscript. The information thus gathered from previous writers is supplemented by extended personal observations by the author in frequent journeys made for the purpose during his residence for a dozen years or more in Japan; and it is these accounts of his own explorations that the ordinary reader will find most interesting. Among them may be especially mentioned his visit to Oshima (p. 78 ff.), where he had an opportunity of looking down into the open crater of an active volcano, which was at the time belching forth masses of molten lava to a height far above the point where he stood. It must certainly have been, as he says, 'a sight of extraordinary grandeur.' A map is given on which are marked 129 mountains of volcanic origin, 23 being in the Kurile Islands. Of these, 51 are active, 16 being in the Kuriles, and 11 in Yezo. Of the whole number, 39 are symmetrically formed cones, showing a more or less close approximation to the theoretical outline deduced by Milne in the *Geological magazine*, and by Becker in the *American journal of science*, and again discussed by Milne in this paper. From several considerations the author infers that the volcanoes of the Kuriles are of more recent formation than those of Japan.

This part, ii., of vol. ix., is issued from the office of the *Japan Mail* in Yokohama, instead of from Tokio as heretofore, and there is also an entire change in its outside appearance. There are numerous errors, which show that the English proof-readers in that office are not yet quite perfect. The word 'ejectamenta' has proved especially puzzling to them, being misprinted in six of the nine places where it occurs.

The Japanese Transactions of this society have reached vol. iii., which contains papers on 'Earth tremors,' by Milne; on the 'Earthquake of Oct. 15, 1884,' by Sekiya; and on 'Air-waves and sea-waves,' by Wada.

The volume recently issued in the International scientific series, on 'Earthquakes,' by John Milne, is also before us; but this article has already reached such a length, that its consideration must be postponed to another time.

C. G. ROCKWOOD, JR.

FROM *Nature* we learn that Japan has thirty-seven periodicals devoted to education; seven of these are medical, with a monthly circulation of 13,514; nine treat of sanitary matters, and two of pharmacy; twenty-nine are what might be termed popular scientific journals, and have a circulation of 70,000.